

Ampère, Electrodynamics, and Experimental Evidence

*By James R. Hofmann**

IN AUGUST 1831 MICHAEL FARADAY discovered that he could produce a temporary electric current in a coiled wire by either starting or stopping another current in a nearby coil. Because of his detection of this induced current and his careful specification of the circumstances in which it had been produced, Faraday soon was credited with the discovery of electromagnetic induction. However, almost nine years earlier André-Marie Ampère had produced a surprising new electrodynamic phenomenon that he attributed to electric current produced by the “influence” of a nearby current in a coiled wire.

It is not my intention to claim priority for Ampère. Although Ampère produced and detected the induction effect in 1822, he did not understand how it was related to the change in the intensity of his primary current. Ampère’s belief that some form of induction persisted as long as he maintained a current in his primary circuit is enough to disqualify him as a claimant to Faraday’s discovery. But, given Ampère’s keen interest in electrodynamics at that time, it seems odd that he did not follow his initial observation with the detailed research that would have clarified the phenomenon.

Ampère’s failure to investigate induction was not simply an unreflective oversight. His reaction to the induction experiment was shaped by the strategy that governed all his most creative research in electrodynamics between 1820 and 1823. This strategy lay behind both his limited and inaccurate study of induction and his far more enthusiastic response to two other experiments he performed on the same day in 1822.

To place these experiments in context, I begin with a general overview of the relevant aspects of Ampère’s early research in electrodynamics. I then argue that this activity was guided by a carefully designed research program that was itself stipulated by a set of fundamental assumptions and a clear-cut heuristic procedure for theory revision. When Ampère argued that his program was superior to those of rival theorists, he particularly emphasized a specific methodological criterion: he insisted that, although theories obviously had to be based upon prior experimental information, the strongest possible confirmatory evidence was the derivation or prediction of additional experimental phenomena that had not been used to create the theory in question.

I next explain the theoretical reasoning and prior experimental circumstances that motivated the three experiments Ampère performed in Geneva with Auguste de La Rive in September 1822. Each of the experiments is intimately tied to

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The present form of this essay owes much to helpful criticisms from J. E. McGuire, L. Pearce Williams, Norton Wise, and James Woodward.

specific problems generated by Ampère's research program. I conclude with a detailed description of Ampère's experiments, his interpretation of their significance, and his utilization of their results. Although Ampère did produce electric current by electromagnetic induction in one of these experiments, the relatively slight importance he attributed to induction phenomena led to his failure to undertake the investigation required for an accurate understanding of this effect. The results of Ampère's other two Geneva experiments entirely overshadowed the induction phenomenon because they were directly relevant to the further development of his program. In particular, the floating-wire experiment provided the type of confirmatory evidence Ampère valued most highly.

Any initial puzzlement about Ampère's indifference to an experimental situation that might have led to a full discovery of induction thus dissolves as we become aware of his strict subordination of experimentation to the predetermined tasks set by a carefully designed research program. Ampère's habitual aversion to exploratory experimentation and his insistence on the mathematical expression of all physical concepts offer a striking contrast to Faraday's more intuitive investigation of complex electromagnetic phenomena and his aversion to mathematical formalism.

I. AMPÈRE'S ELECTRODYNAMICS: 1820–1822

As late as 1822 and 1823 lively debate continued as to whether the long-standing conception of electricity and magnetism as disjoint domains of natural phenomena should be replaced by Ampère's new demarcation between electrostatics and electrodynamics. The controversy had been initiated in 1820 when H. C. Oersted discovered that an electric current could be used to alter the natural orientation of a suspended bar magnet. Shortly thereafter Ampère had discovered that attractive and repulsive interactions take place between metallic wires when they carry electric currents;¹ from these observations he argued that a sharp distinction between the domains of magnetic and electric phenomena could no longer be maintained. Instead, Ampère proposed that all magnetic phenomena could be attributed to the presence of electric currents within magnetized bodies; that is, magnetic forces are actually due to complicated combinations of the same "electrodynamic" forces between electric currents that Ampère had discovered using conducting wires. Inspired by this conviction, Ampère set out to give his idea a succinct and elegant mathematical formulation.

When Ampère began his revolutionary theorizing in 1820, he was confronted by a very influential group of established scientists who fully intended to incorporate the new, allegedly "electrodynamic" phenomena within the domain of their

¹ For a detailed investigation of Ampère's discovery, see L. Pearce Williams, "What Were Ampère's Earliest Discoveries in Electrodynamics?," *Isis*, 1983, 74:492–508. For some objections and Williams's reply, see Christine Blondel, "Ampère and the Programming of Research," *Isis*, 1985, 76:559–561; and L. Pearce Williams, "Reply," *Isis*, 1985, 76:561. The best general survey of Ampère's electrodynamics is Christine Blondel, *A.-M. Ampère et la création de l'électrodynamique (1820–1827)* (Paris: Bibliothèque Nationale, 1982).

² The best introduction to Laplacian physics is still Robert Fox, "The Rise and Fall of Laplacian Physics," *Historical Studies in the Physical Sciences*, 1974, 4:89–136. Two other valuable sources for the Laplacian context of Ampère's research are Rod Home, "Poisson's Memoirs on Electricity: Academic Politics and a New Style in Physics," *British Journal for the History of Science*, 1983, 16:239–259; and E. Frankel, "J. B. Biot and the Mathematization of Experimental Physics in Napoleonic France," *Historical Studies in the Physical Sciences*, 1977, 8:33–72.

own research program in magnetism. By 1816, when Jean-Baptiste Biot published his authoritative *Traité de physique expérimentale et mathématique*, four distinct imponderable fluids had become the foundations for what Robert Fox has called the "Laplacian" sciences of electricity, heat, optics, and magnetism.² Moreover, despite the objections that Augustin Fresnel, Dominique Arago, and Jean Fourier were raising against corpuscular optics and the caloric theory, electricity and magnetism remained Laplacian strongholds throughout the decade prior to 1820. Only at that point did Oersted's discovery instigate the revolutionary research through which Ampère eventually delivered the coup de grâce to Laplacian physics. Throughout the early 1820s conservative resistance by Biot, Simon-Denis Poisson, and Claude Pouillet placed Ampère in a vigorously competitive environment.

Ampère's initial concern was to investigate the interactions he had discovered between conducting wires in the hope of inferring an expression for an electrodynamic force between any pair of circuit segments small enough to be considered "infinitesimal" in comparison to measurable magnitudes.³ Once the relevant mathematical integrations were carried out, the formula could be applied to any imaginable electric circuitries, including those Ampère believed to exist within magnets; it thus would constitute the basis of a mathematical representation of the forces that, according to Ampère, defined the electrodynamic domain.

As early as December 1820 Ampère had assessed his initial observations and concluded that the most general expression for the electrodynamic force between two tiny current elements, ds and ds' , should be of the form

$$r^{-n}(\sin \alpha \cdot \sin \beta \cdot \cos \gamma + k \cdot \cos \alpha \cdot \cos \beta) \cdot i \cdot i' \cdot ds \cdot ds'. \quad (1)$$

In this expression the current strengths are i and i' ; the two current elements are separated by a distance r , and the force is directed along the line joining them. Also, as shown in Figure 1, the current elements make angles α and β with the line joining them, and γ is the angle between the two planes, each of which contains the connecting line and one of the two current elements. That is, α , β , γ , and r are all variables depending upon the relative positions and orientations of the two current elements. On the other hand, n and k are numerical constants that would eventually have to be assigned specific values in order for applications of the formula to yield quantitative empirical predictions.⁴

Throughout the first two years of his research Ampère relied upon analogy to the inverse square laws of gravity and electricity as justification for the tentative assumption that $n = 2$; during the first eighteen months of this period he also interpreted some very limited experimental evidence to conclude that $k = 0$.⁵ The latter inference entirely eliminated the second term from Ampère's formula

³ For a valuable commentary and an edited transcription of manuscript sources pertaining to Ampère's early research on his force law, see Christine Blondel, "Sur les premières recherches de formule électrodynamique par Ampère (octobre 1820)," *Revue d'histoire des sciences et de leurs applications*, 1978, 31:53–65.

⁴ Ampère presented his force law to the Académie Royale des Sciences on 4 Dec. 1820; for his earliest published report see A.-M. Ampère, "Note sur un mémoire lu à l'Académie royale des Sciences, dans la séance du 4 décembre 1820," *Journal de Physique, de Chimie, d'Histoire Naturelle et des Arts*, 1820, 91:226–230.

⁵ The first few months of Ampère's research on this project are analyzed in James R. Hofmann, "Ampère's Invention of Equilibrium Apparatus: A Response to Experimental Anomaly," *Brit. J. Hist. Sci.*, 1987, 20.

and reduced his expression for the electrodynamic force to a quantity proportional to

$$r^{-2} \sin \alpha \cdot \sin \beta \cdot \cos \gamma \quad (2)$$

It was not until April 1822 that Ampère used important new experimental evidence to determine that this conclusion was incorrect and that $k = -\frac{1}{2}$.⁶

Simultaneously with this work, Ampère was pursuing a much more ambitious project. Ultimately he hoped to demonstrate that all magnetic and electric phenomena are the consequences of oscillatory motions propagated within an ethereal fluid of particles made up of superpositions of positive and negative electric fluids. In fact, by the end of 1820 Ampère was already thinking of an electric current as a "chain" of short-lived polarizations of the two electric fluids constituting the ethereal atmospheres of molecules within the conducting body. Soon thereafter he began sketching some preliminary ideas concerning the spatial transmission of electrodynamic forces by means of a transverse propagation of a periodic state of electric polarization within the ether particles surrounding each electric current.⁷ The rationale behind Ampère's response to new experimental evidence is his constant concern with linking the quantitative properties of his speculative ether model to his more empirically constrained formulation of the electrodynamic force law.

Meanwhile, two important preliminary projects dominated Ampère's early research. He needed a rigorous determination of the values of the two parameters n and k , and he wanted to stipulate the circuitries followed by electric currents within magnetized needles and bar magnets.⁸

Concerning the second issue, Ampère initially only speculated that the currents responsible for magnetism should encircle the magnetic axis within planes perpendicular to that axis; he did not stipulate the size of these circuits.⁹ In January 1821, however, Ampère responded to criticism from Fresnel by tentatively concluding that magnetic circuits must be restricted to individual molecules. By adopting this molecular theory, Ampère sought to avoid Fresnel's objection that thermal effects should be detectable within a magnet if its magnetism is due to electric circuits of macroscopic dimensions.¹⁰

⁶ It is important to understand the sense in which there is only one "correct" expression for Ampère's formula, namely, the version in which the parameters have the values $n = 2$ and $k = -\frac{1}{2}$. Since Ampère believed his force acted along the line joining the two circuit elements, he preserved the application of Newton's third law stating the equality of action and reaction along that line. Modern physicists are more accustomed to basing their electrodynamics upon the so-called Biot-Savart law for the magnetic field produced by a current element, a procedure for which Newton's third law does *not* apply. Nevertheless, using the above values for the parameters n and k , Ampère's formula provides the same results as the Biot-Savart law whenever the two procedures are applied to *closed circuits*; for a proof of this equality, see R. A. R. Tricker, *Early Electrodynamics: The First Law of Circulation* (London: Pergamon, 1965), pp. 55–58. Since steady currents can be maintained only in closed circuits, Ampère's formula is quite capable of serving as a basis for the explanation of all steady-state closed-circuit phenomena.

⁷ For a discussion of Ampère as an anti-Laplacian ether theorist and a valuable translation of a relevant text, see Kenneth Caneva, "Ampère, the Etherians, and the Oersted Connection," *Brit. J. Hist. Sci.*, 1980, 13:121–138.

⁸ For a good survey of this research see Blondel, *Ampère* (cit. n. 1), Chs. III, IV.

⁹ A.-M. Ampère, "Analyse des mémoires lus par M. Ampère à l'Académie des Sciences dans les séances des 18 et 25 septembre, des 9 et 30 octobre 1820," *Annales Générales des Sciences Physiques*, 1820, 6:238–257, on p. 242.

¹⁰ Augustin Fresnel, "Comparaison de la supposition des courants autour de l'axe avec celle des courants autour chaque molécule," and Fresnel, "Deuxième note sur l'hypothèse des courants par-

In September 1820 Ampère had been equally responsive to Arago's discovery that a metallic needle can be magnetized by sending an electric current through a helical wire wrapped around the needle.¹¹ Two explanations could be given. One possibility was that a needle might become magnetic owing to the production of electric currents within it when exposed to the surrounding helical current. But it was also conceivable that these electric currents might exist prior to magnetization; the needle might simply become magnetic when an originally disordered collection of molecular currents takes a uniform orientation in response to the forces exerted by the surrounding helical current. In October 1820 Ampère demonstrated how the orientation of large suspended circular circuits could be

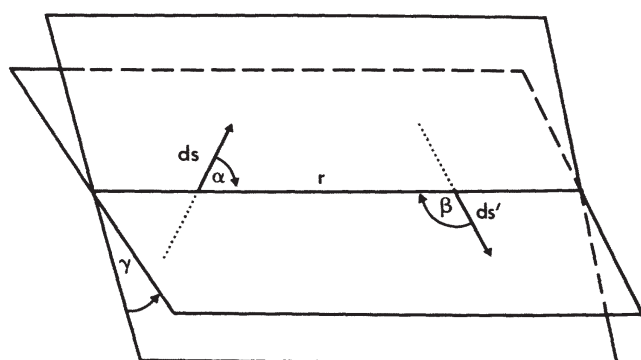


Figure 1. The geometry of Ampère's force law.

changed by exposing them to other appropriately positioned currents; this phenomenon supported the uniform alignment explanation of Arago's magnetization process.

In addition Ampère performed an experiment in July 1821 that, as he put it, "completely fixed my opinion" in favor of the uniform alignment explanation.¹² Using essentially the same experimental design he would employ in Geneva in 1822, Ampère unsuccessfully attempted to detect electric current in a circular strip of copper while exposing it to a current in a closely adjoining coil. This led Ampère to conclude at the time that because he could not produce electric current by exposing conductors to other currents, the currents he presumed to exist within a magnetized needle must have existed there prior to the magnetization of the needle. That is, Arago's magnetization technique simply gave a uniform orientation to electric currents that had previously produced no observable effects because of the mutual cancellations brought about by their random orientations.

Ampère also claimed that this conclusion in its turn implied "indirectly" that the currents responsible for magnetic effects were of strictly molecular dimensions; he apparently felt that even randomly oriented macroscopic currents should produce detectable effects. Ampère was justifiably dissatisfied with this

ticulaires," both in *Mémoires sur l'électrodynamique*, ed. J. Joubert, Vols. II and III of *Collection de Mémoires relatifs à la physique*, 5 vols. (Paris: Société de Physique, 1885–1887) (hereafter *Collection*), Vol. II, pp. 141–147. Ampère's first published discussion of the molecular theory is his "Exposition du moyen par lequel il est facile de s'assurer directement, et par des expériences précises, de l'exactitude de la loi des attractions et répulsions des courans électriques, suivie de quelques observations sur cette loi. Mémoire lu le 26 décembre 1820," *Annales des Mines*, 1820, 5:553–558, on pp. 557–558.

¹¹ François Arago, "Expériences relatives à l'aimantation du fer et de l'acier par l'action du courant voltaïque," *Annales de Chimie et de Physique*, 1820, 15:93–102, on p. 96.

¹² A.-M. Ampère, "Réponse de M. Ampère à la lettre de M. Van Beck [sic] sur une nouvelle expérience électro-magnétique," *Journal de Physique*, 1822, 93:447–467, on p. 447; reprinted in *Collection*, Vol. II, pp. 212–237. (This and all translations are mine.)

argument, and he maintained a vigilant search for more compelling confirmation of his molecular theory. As a happy bonus, this project brought about the unexpected discovery of the correct value for the parameter k in his force formula.

This important discovery was the result of an intense period of research beginning in October 1821, when Ampère learned of Faraday's first production of some of his famous "rotary effects," such as the uniform rotation of a conducting wire suspended above the pole of a bar magnet.¹³ Ampère initially hoped that an analysis of rotary phenomena would provide a conclusive argument for his molecular theory of magnetism. Although this project was not successful, by April 1822 it had inspired Ampère to carry out a brilliant mathematical analysis of an experiment that had provided results contrary to his expectations. This analysis yielded the relationship $2k = 1 - n$ for the two parameters in his force law. Ampère's assumption that $n = 2$ meant that $k = -\frac{1}{2}$ instead of being of negligible magnitude. The revised version of his expression for the force between two current elements thus became proportional to

$$r^{-2}(\sin \alpha \sin \beta \cos \gamma - \frac{1}{2}\cos \alpha \cos \beta). \quad (3)$$

This revision of the most important force law of Ampère's electrodynamics clearly was a major turning point in his research; two of the experiments he performed in Geneva in September 1822 were designed to confirm it. But in order to understand Ampère's assessment of these experiments and the accompanying repetition of his induction experiment, we must appreciate how he tailored his experimental strategy to a framework of goals specified by a tightly structured research program.

II. AMPÈRE'S ELECTRODYNAMIC RESEARCH PROGRAM

The foregoing summary of Ampère's early research highlights only a very limited sample from the complex set of new phenomena he confronted. That Ampère was able to weave these details into a cohesive fabric was due to an astute choice of a particularly fruitful mode of explanation, a "research program" that guided his search for an increasingly trustworthy theory of electrodynamics—a program that included a heuristic guideline for theory revision.¹⁴

First, Ampère adopted a set of fundamental guiding assumptions specifying the conceptual framework within which electrodynamic phenomena were to be explained. In this respect Ampère advocated a revolutionary realignment of the domains of early nineteenth-century French physics. He did not directly challenge the well-established electrostatics of Charles Coulomb, Poisson, and Biot, based on an ontology of two electric fluids; however, he called for an outright rejection of the magnetic fluids that had been presumed by the corresponding program in magnetism. That is, the central postulate of Ampère's program was that all electrodynamic phenomena, including all magnetic effects, are due to

¹³ Michael Faraday, "On some new Electro-Magnetical Motions, and on the Theory of Magnetism," *Quarterly Journal of Science*, 1821, 12:74–96; and Faraday, "Sur les Mouvements électromagnétiques et la théorie du magnétisme," *Ann. Chim. Phys.*, 1821, 18:337–370.

¹⁴ On research programs see Imre Lakatos, "History of Science and its Rational Reconstructions," and Ian Hacking, "Lakatos's Philosophy of Science," both in *Scientific Revolutions*, ed. Ian Hacking (London: Oxford Univ. Press, 1981). In the present essay I neither utilize nor test the full detail of Lakatos's methodology; I have kept metatheoretical terminology to a bare minimum as my sole intention here is to clarify Ampère's views on scientific progress and theory appraisal.

interactions that arise between particles of the electric fluids as a result of the motions they undergo within electric circuits. The ultimate explanatory task of a theory of electrodynamics was to give a mathematical formulation of these interactions compatible with Newtonian dynamics, such that all measurements of electrodynamic phenomena are accurately approximated by calculated consequences of the theory.¹⁵

Several important qualifications must be noted to avoid a misconstrual of these assumptions. First, Ampère did not stipulate whether the electrodynamic interactions between electric fluid particles were necessarily analogous to action-at-a-distance electrostatic forces, or whether they might have a restricted range pertaining only to directly adjacent pairs of particles. During his career Ampère in fact considered both of these possibilities.¹⁶

Second, Ampère interpreted the restraint of Newtonian dynamics to mean that any mathematical formulation of a force acting between a pair of electric fluid particles must not only conform to the general statement of Newton's third law but must also be a function solely of the distance between the particles and directed along the line joining them. This requirement does not rule out the possibility that, owing to the specific directions of the oscillations of electric fluid particles within electric currents, more complicated force expressions might be needed to represent the composite interactions pertaining to "infinitesimal" segments of an electric circuit. Indeed, Ampère's famous force law turned out to be a complicated function of the mutual orientations of two current elements, elements that are "infinitesimal" only in comparison to measurable magnitudes and that actually contain multitudes of oscillating fluid particles.

Third, a full execution of the explanatory task set by the assumptions of Ampère's program required a detailed stipulation of the electric fluid motions that give rise to the phenomena attributed to electric currents. Ampère accepted this task in conjunction with his commitment to an all-pervasive luminiferous ether. In fact, Ampère believed space itself to be a noncorporeal substance made up of ether particles that, in their undisturbed neutral state, consist of superpositions of equal quantities of the two oppositely charged electric fluids. Furthermore, he considered the molecules of each chemical element to have a characteristic electric charge that ordinarily is neutralized by a surrounding ethereal atmosphere of an appropriately opposite net charge. Early in 1821 Ampère was arguing that the transmission of an electric current through a conductor actually results from the periodic creation of a "chain" of nearly simultaneous separations of the two electric fluids within each molecular atmosphere, followed almost immediately by their collective return to the initial equilibrium condition; he thus denied any actual "flow" of either of the electric fluids through the entire circuit.¹⁷

Although Ampère continued to develop his conception of electric current

¹⁵ Ampère's clearest summary of this project is found in his "Mémoire sur la théorie mathématique des phénomènes électrodynamiques uniquement déduite de l'expérience . . .," *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, 1827, 6:175–388, on pp. 293–302; reprinted in *Collection*, Vol. III, pp. 1–193; see pp. 112–117. This was a slightly revised version of his November 1826 publication *Théorie des phénomènes électro-dynamiques, uniquement déduite de l'expérience* (Paris, 1826).

¹⁶ Ampère, "Mémoire sur la théorie mathématique," *Collection*, Vol. III, pp. 116–117, 137, 171.

¹⁷ For Ampère's first extensive discussion of electric current, see his "Réponse" (cit. n. 12). He gave a much more sophisticated presentation in his notes for a physics course at the Collège de France; these have recently been edited in Blondel, *Ampère* (cit. n. 1), pp. 177–186.

throughout his career, he never was able to make it precise by providing a quantitative expression for moving electric fluid particle interactions. A similar fate awaited his speculations that attractive and repulsive interactions between conductors of electric current might arise from the transverse propagations of a state of polarization within the electric fluids constituting particles of the surrounding ether. Only if Ampère had been able to provide an empirically adequate mathematical expression for all these highly complex processes would he have accomplished the full explanatory task set by the guiding assumptions of his program.

That this goal eluded Ampère should not tempt us to conclude that it was detrimental or, at best, only incidental to the more modest success he did achieve. Ampère was convinced that the ultimate task of science was the reduction of all phenomena to the real properties of a primitive ontology, a bedrock of objective truth. Through an idiosyncratic, but systematic, misuse of Kantian terminology he referred to this base as the “noumenal” reality responsible for the world of phenomena.¹⁸ In this respect Ampère’s program provided an indispensable psychological incentive. Only his conviction that electrodynamics gave new access to the noumenal realm could generate the energy he expended, at the relatively advanced age of forty-five, in his first extensive foray into theoretical physics. The conceptual demands imposed by Ampère’s program directly inspired both his most creative experimentation and his most creative theoretical responses to experiment. While so many other experimenters wandered aimlessly in a labyrinth of empirical details, Ampère’s research was snugly harnessed to theory construction.

For example, in 1820 Ampère made the following comment concerning a case in which his theory and Biot’s had different implications but were not experimentally distinguishable:

Les théories ne peuvent sans doute être complètement démontrées que par l’accord du calcul et des observations, mais ce n’est que par leur secours que nous pouvons prévoir d’avance la forme des fonctions, et trouver les valeurs qu’il convient de calculer pour les comparer aux données de l’expérience. Sans la théorie que j’ai déduite de la loi que je communiquai à l’académie le 6 novembre dernier, comment aurait-on, par exemple, été conduit à essayer de comparer aux mesures fournies par les observations la fonction $\frac{2}{3} (\sin b + \operatorname{cosec} b - \cot b)$ plus tôt que toute autre?¹⁹

Ampère’s remark calls our attention to the second aspect of his research program, the one to which he owes much of his fame as a physicist. During the first few weeks of his research Ampère designed a heuristic strategy for the creation and revision of increasingly trustworthy theories conforming to the constraints of his general explanatory assumptions. That is, lacking any quantitative ether mechanics for the electric fluids, Ampère shouldered the preliminary task of showing that all electrodynamic phenomena could be explained in terms of forces arising from either macroscopic electric currents or their unobservable counterparts within magnets. In this context the microstructure of electric current was

¹⁸ Ampère takes up this topic repeatedly in his correspondence and unpublished manuscripts; see, e.g., Ampère to F.-P. Maine de Biran, 14 octobre 1805, in *La correspondance du Grand Ampère*, ed. L. de Launay, 3 vols. (Paris: Gauthier-Villars, 1936–1943) (hereafter *Correspondance*), Vol. I, p. 286.

¹⁹ Ampère, memoir presented to the Académie des Sciences on 8 Jan. 1821, scribe’s copy, Archives de l’Académie des Sciences, Box 8, Folder 166.

not specified beyond the claim that it consisted of moving electric fluids; current direction was defined operationally in terms of the experimental apparatus. The successful implementation of Ampère's heuristic strategy thus would preserve the plausibility of the central ontological assumption of his program while generating increasingly accurate theories couched in terms of electric currents.

We have already seen how the heuristic component of Ampère's program dictated a straightforward twofold procedure. First, on the basis of the experimental study of electric circuits, an electrodynamic force law for pairs of circuit elements should be discovered; this law should then be assumed to be applicable not only to large-scale circuits but also to the unobservable circuitries of magnets. This law might have to be revised repeatedly in response to new experimental evidence, but in no case should a search for an empirically adequate version be abandoned. For example, when Ampère used the result of the experiment he performed early in 1822 to conclude that the correct value of the parameter k was $-\frac{1}{2}$ rather than zero, he was making a major theory revision within the relatively flexible structure of his research program.

Second, the stipulation of the geometry of magnetic circuitries should be pursued in an effort to demonstrate that all magnetic phenomena were consequences of the electrodynamic force law. Thus, as will be discussed in detail shortly, Ampère argued that some important experimental observations by Faraday and Gaspard de La Rive could best be explained by assuming that molecular electric currents within bar magnets were inclined to the axis of the magnet at angles that increased with the distance of the molecules from the axis. Once again, since Ampère originally had supposed that all magnetic circuits were perpendicular to the axis, his new stipulation of magnetic circuitries represented an important theory revision within his program.

At this point it is natural to wonder whether Ampère's contemporaries objected that these revisions are illegitimately *ad hoc*. In fact Ampère's close friend Frédéric Maurice wrote in July 1822 to warn him that a scientific acquaintance, who preferred to remain anonymous, found Ampère's electrodynamics difficult to accept. Maurice quoted his well-informed correspondent's claim that Ampère's explanations were flawed owing to

le grand nombre d'hypothèses toutes gratuites, l'abus de la considération des infiniment petits avec lesquels on peut dire tout ce qu'on veut, et le mélange de certaines idées dynamiques dont l'introduction n'est pas suffisamment motivée ni l'influence nettement caractérisée. Il paraît même que l'auteur [Ampère] a fait récemment quelques modifications à son hypothèse sur la disposition des courants dans les barreaux aimantés, afin de pouvoir expliquer certain phénomène observé par M. de La Rive.

Ampère's reply to Maurice reveals his unflinching commitment to the guiding assumptions and heuristic strategy of his program:

J'espère bien que ce ne sera pas le dernier de ces changements de détail que j'y apporterai, car les théories de ce genre ne se perfectionnent qu'avec le temps. Aurait-il fallu rejeter la théorie de l'attraction parce que la précession des équinoxes ou l'aplatissement de la Terre donnés par l'observation était [*sic*] moindre que ce que donnait le calcul en supposant la Terre homogène? Non, mais il fallait dire: la Terre n'est pas homogène. C'est l'observation de M. Faraday sur ce que les pôles des hélices sont exactement à leurs extrémités et non pas ceux des aimants, qui m'a donné cette idée de l'inclinaison des courants des particules qui ne sont pas dans

l'axe. Mais il est vrai que c'est l'expérience de M. de La Rive qui m'a confirmé dans cette vue sur la situation des courants de l'aimant. On trouve ainsi une différence entre les aimants et les hélices. Faut-il dire: *les aimants ne doivent pas leur action à des courants électriques*? Non, mais il faut dire: ces courants sont inclinés sur l'axe des aimants là où ils doivent être en vertu de l'action mutuelle de tous les courants de l'aimant. Voilà, je le répète, la marche à suivre dans les sciences où chacun peut perfectionner ce qu'ont fait les autres.²⁰

Ampère clearly realized that the *ad hoc* charge carried considerable weight among colleagues who had recently witnessed Fresnel and Arago dismantle Biot's optics with the same objection; the charge had to be met at the methodological level. This is why Ampère emphasized that his strategy of theory revision had a clearly discernable *marche*. Each revision had to conform to the constraints of his guiding assumptions and the application of his force law. More importantly, however, Ampère also emphasized a specific methodological criterion for the appraisal of rival programs. That is, an important sign that a research program is worth pursuing is the fact that the theories it generates entail observable phenomena that were not relied upon to create the theories in question.²¹ I use the term *entail* here to include both predictions of hitherto unobserved phenomena and the *post hoc* explanation of those already known. My intent is to mirror Ampère's own use of the term *prédiction*. In the early stages of his career Ampère demanded strictly temporal prediction; he adopted a more liberal entailment criterion in the heat of actual scientific practice.

For example, in some 1801 notes containing his earliest speculations about electricity Ampère had claimed that in proposing a *système physique* as a causal foundation for a domain of phenomena, "on a d'autant plus de moyens de la vérifier, qu'on en a moins employé à la trouver. On n'a qu'à imaginer de nouvelles expériences et en prédire l'événement. Il suffit ensuite de les tenter pour voir si elles confirment ou non les conjectures qu'on avait faites."²² In this early passage Ampère seems to have had in mind a strictly temporal sense of prediction in which the most valuable confirmation of a theory would be to predict phenomena that had never been known to occur.

However, by 1816 Ampère had liberalized this very strict requirement. In that year he wrote that when a scientist is confronted by a choice between alternative theoretical explanations, he should "n'adopter définitivement celle qui paraîtrait la plus probable qu'après l'avoir vérifiés en en tirant des conséquences qui en soient une suite nécessaire, et qui se trouvent confirmées par des faits auxquels on n'avait point eu recours dans les recherches qui y ont conduit, et que ces conséquences mêmes indiquent à l'observateur dans le cas où il ne les aurait pas encore remarqués."²³ Although the qualification appended in the last phrase of this passage is not completely clear, Ampère seems to be emphasizing the fact

²⁰ Frédéric Maurice to Ampère, 5 July 1822, in *Correspondance*, Vol. III, p. 924; and Ampère to Maurice, 6 July 1822, *ibid.*, p. 927.

²¹ Ampère's criterion is a striking example of the methodology presented in John Worrall, "The Ways in Which the Methodology of Scientific Research Programmes Improves on Popper's Methodology," in *Progress and Rationality in Science*, ed. Gerard Radnitzky and Gunnar Anderson (Boston Studies in the Philosophy of Science) (Dordrecht: Reidel, 1978), pp. 45–70. On Fresnel and Arago's strategy see E. Frankel, "Corpuscular Optics and the Wave Theory of Light: The Science and Politics of a Revolution in Physics," *Social Studies of Science*, 6:141–184.

²² Ampère, "Reserches sur les causes de l'attraction et de la répulsion électrique," undated MS (1801?), Arch. Acad. Sci., Box 10, Folder 205 bis.

²³ Ampère, "Traité de psychologie," Arch. Acad. Sci., Box 17, Folder 269.

that decisive evidence is provided by all entailed facts that were not used to create the theory in question; temporal prediction of new phenomena is no longer claimed to be essential.

Fortunately, we need not rely solely upon an exegesis of these texts to understand Ampère's methodology; in his actual scientific practice in electrodynamics he repeatedly cited experimental evidence in ways that demonstrate his priorities. For example, consider Ampère's response to the extensive electrodynamic discoveries that overwhelmed him during months of serious illness early in 1821. Although he had provided no theoretical prediction of these new effects, he welcomed them as strong confirmatory evidence because his latest theory "aurait pu servir à les prévoir d'avance."²⁴ In other words Ampère could have predicted these phenomena had he been quick-witted enough to apply his most recent theory to the relevant initial conditions.

Similarly, writing to Paul Erman in April 1821, Ampère exclaimed: "Les observations décrites dans le mémoire que vous avez eu la bonté de m'envoyer en sont autant de nouvelles preuves; car, si je ne me trompe, on les pouvait toutes prévoir d'après la théorie où l'on considère les aimants comme des assemblages de ce que j'appelle courants électriques."²⁵ Once again, Ampère is arguing that Erman's discoveries are valuable *preuves* of his electrodynamic program because they were not used to create the theory that could have predicted them in advance. By emphasizing the nontemporal logical relationship between theory and observational data, Ampère tried to defuse any potentially unfavorable psychological response elicited by new phenomena that he had not been able to predict.

Ampère's view should certainly not be misunderstood as a claim that this is the *only* significant form of confirmatory evidence. In other contexts he stressed sheer explanatory scope and conceptual simplicity as important factors in the appraisal of research programs.²⁶ Nevertheless, during the first hectic years of his most creative research, there were some intensely competitive periods when the criterion of successful "prediction" loomed large. This was particularly true prior to 1823; recognition of this emphasis thus becomes indispensable for understanding the priorities Ampère applied in his choice of experiments and his assessment of their results. In particular it explains why, unlike Faraday, for example, Ampère rarely engaged in exploratory experimentation in search of new phenomena. By early 1821 the development of his research program had already sharply focused his experimentation and had become the reference frame for assessing the value of his results.

III. THE CONTEXT FOR AMPÈRE'S 1822 GENEVA EXPERIMENTS

We should expect that the experiments Ampère performed in Geneva in September 1822 grew out of issues generated by the guiding assumptions and theory revisions of his research program. Also, we should expect that Ampère's assessment of the importance of his results would conform to his customary assignment of highest value to those phenomena entailed by his latest theory but not used to

²⁴ A. Ampère, "Lettre de M. Ampère à M. Arago," *Ann. Chim. Phys.*, 1821, 16:119–129, on p. 128.

²⁵ Ampère to Paul Erman, 20 Apr. 1821, in *Correspondance*, Vol. III, p. 917.

²⁶ See, e.g., A. Ampère, *Recueil d'observations électro-dynamiques* (Paris: Crochard, 1822), pp. 354–364, rpt. in *Collection*, Vol. II, pp. 376–383. (Although this edition bears the publication date 1822, it is actually an expanded version of the true 1822 edition, and was published in 1823. For details see Williams, "What Were Ampère's Earliest Discoveries" [cit. n. 1], p. 494, n. 8.)

create the theory. The first of these expectations is fully confirmed by a consideration of how Ampère's program in 1822 shaped the specific issues each experiment was designed to resolve or clarify. Let us begin with Ampère's investigation of induction.²⁷

The issue arose during the first few weeks of Ampère's research in September and October 1820. In one of his earliest résumé articles Ampère claimed that whenever the two electric fluids are decomposed or polarized at a point on a metallic surface,

les deux électricités seront portées en sens contraire sur cette surface, et iront se réunir sur sa face opposée par la courbe fermée qui aura le moins de longueur, ce qui établira un courant suivant cette courbe. Ce courant détermine dans les molécules de l'acier la disposition analogue à celle des éléments d'une pile voltaïque qui tend à le continuer, et entraîne des courants, et par suite des dispositions semblables dans le reste du corps.²⁸

Ampère thus believed that one current within a metallic body could "pull along" (*entraîner*) another current in an adjacent region.

Similarly, both he and Arago applied this idea in their description of the magnetization of a needle or wire when it is placed inside a helical circuit wrapped around an insulating tube of glass: "Le courant de chaque spire en entraîne un semblable et dirigé dans le même sens sur la surface de l'acier, et par suite d'autres dans son intérieur. C'est pour la même raison que l'aimantation s'étend au-delà de l'hélice quand il est plus long qu'elle."²⁹ In both of these cases the claim is that one *steady* current apparently can "entraîner" another such current. No effects are attributed to sudden changes in currents.

In November 1820 Ampère and Fresnel attempted to reverse this magnetization procedure to produce a current in a wire wrapped around a bar magnet. After rectifying an erroneous interpretation of some preliminary observations, they agreed that they could not produce currents using stationary magnets; they did not investigate the possibility of effects caused by putting these magnets in motion.³⁰

For most of 1820 Ampère seems to have assumed that the currents within bar magnets were centered on the axis of the magnet within planes normal to the axis. By January 1821 Fresnel had convinced him that molecular currents in these planes would be a more reasonable hypothesis. The existence of large-scale currents implied effects that were not observed experimentally, such as the production of heat. Ampère's new molecular theory in turn allowed for two alternative amplifications. Magnetization might be brought about either through the "production" of molecular currents or through a uniform orientation of previously existing randomly oriented molecular currents. Ampère had apparently held a "production" view in 1820; the new molecular theory made him reexamine this question. Several developments early in 1821 set the stage for his next attempt to detect induced current.

²⁷ *Induction* is a term that Ampère did not use until Faraday began doing so following his own discovery in 1831. Before that time Ampère typically discussed the question in terms of whether electric current could be "excited" or "produced" by the "influence" of another current.

²⁸ Ampère, "Analyse" (cit. n. 9), p. 249.

²⁹ *Ibid.*, p. 250.

³⁰ A. Fresnel, "Note sur des essais ayant pour but de décomposer l'eau avec un aimant," *Ann. Chim. Phys.*, 1820, 15:219–222.

First, Fresnel continued to espouse the molecular hypothesis, and he offered an impressive list of favorable experimental evidence in drafts for two short memoirs. Although these were never published by Fresnel, they surely were known to Ampère since they are found among his papers in the archives of the Académie des Sciences. One of them is dated 5 June 1821, and together they probably represent a summary of the arguments Fresnel had proposed to Ampère during the preceding few months. In particular, Fresnel claimed that the molecular and macroscopic theories had different implications inasmuch as steel cylinders are magnetized by surrounding them with helical currents. Assuming that magnetization comes about because the spires of the helix produce either molecular or coaxial currents on the surface of the cylinder parallel to the spires, straight currents should also produce either molecular or large-scale currents in an adjacent linear conductor. Fresnel's description of the two cases may very well have inspired Ampère's induction experiment in July 1821. If all currents involved are macroscopic, "le courant d'un fil conjonctif produirait un autre courant dans un fil d'acier parallèle au premier, et dont les extrémités seraient mises en communication par un conducteur." On the other hand,

dans la théorie des courants autour de chaque molécule, un fil conjonctif parallèle à un fil d'acier doit produire autour des molécules de celui-ci des courants dans le plan passant par les deux fils. Il n'y aura pas de courant longitudinal; des grenouilles interposées entre les deux extrémités de ce fil ne seront pas affectées; l'eau ne sera pas décomposée, etc. Le fil d'acier ne présentera ensuite aucun signe de magnétisme.³¹

Fresnel thus suggested that a failure to produce electrodynamic effects in a conductor by exposing it to a parallel current would be strong evidence for the molecular theory. This was the experiment Ampère performed for the first time in July 1821; to amplify the action of his primary current, he wrapped it into a vertical coil and suspended a circular conductor directly inside it.

Several familiar pieces of equipment contributed to Ampère's experimental design. First, during his initial demonstrations of the influence of terrestrial magnetism upon current loops Ampère had often suspended such a loop using a thread or wire. He found that the loop did not quite rotate fully so as to be normal to the earth's magnetic meridian. He attributed this to a resisting torsion in his suspension, which he later rectified by switching to a mercury-lubricated pivot suspension.³² However, in both the 1821 and 1822 induction experiments Ampère returned to wire or thread suspensions, and his assessment of the torsion factor became an important part of his interpretation of his 1822 results.

Second, an interesting experiment by Erman caught Ampère's eye when it was reported in the *Bibliothèque Universelle* early in 1821. In a published letter to Erman dated 20 April 1821 Ampère pointed out that from the electric theory of magnetism, it is obvious that "les deux branches d'un aimant en fer à cheval doivent attirer ou repousser ensemble un même conducteur voltaïque vertical. . . . Il faut seulement se rappeler que l'attraction a lieu quand les courans de l'aimant, dans le partie le plus voisine du conducteur, sont dans le même sens

³¹ Fresnel, "Comparison," quoting p. 141; see also Fresnel, "Deuxième note" (both cit. n. 10).

³² Ampère, "Analyse" (cit. n. 9), pp. 252–254; and A.-M. Ampère, "Suite du Mémoire sur l'Action mutuelle entre deux courans électriques, entre un courant électrique et un aimant ou le globe terrestre, et entre deux aimans," *Ann. Chim. Phys.*, 1820, 15:170–218, on p. 190.

que celui du fil conjonctif, et la répulsion quand ils sont en sens contraire.” Erman had used a bar magnet detector in his own experiment; Ampère would use bar magnets in 1821 and a more powerful horseshoe magnet in 1822. Similarly, in March 1821 Gaspard de La Rive had used a bar magnet to demonstrate attractions and repulsions of a vertical current loop.³³ Consequently, by early 1821 Ampère was thoroughly familiar with apparatus in which *steady currents* in vertical current loops were rotated using magnets. Fresnel’s suggestive arguments concerning molecular currents in magnets then became the catalyst for Ampère’s invention of his July 1821 induction apparatus.

As shown in Figure 2, Ampère suspended a circular copper loop, *HIG*, just within the circumference of a stationary circular coil of copper wire, *DEB*. A small gap separates the suspended loop from the stationary coil; when a current was sent through the coil, *DEB*, it became the primary circuit for Ampère’s attempt to induce current within the suspended loop.

Writing to Albert van Beek early in 1822, Ampère recalled that the arrangement he had chosen “m’a paru la plus convenable pour exciter dans ce cercle des courants électriques par influence, si cela était possible; mais, en le présentant à l’action d’un fort aimant, je n’ai pas aperçu qu’il prit aucun mouvement, malgré la grande mobilité de ce genre de suspension.”³⁴

Unfortunately Ampère left no clues concerning how he manipulated his primary current and what variables he considered relevant. No doubt he was expecting to produce steady current, if any, and he expected this to be revealed by a rotation of the copper loop due to a repulsion or attraction by the bar magnets he had placed in fixed positions on the platforms *ng* and *kp*.

The initial conclusions Ampère drew from his experiment are recounted in a letter to van Beek written between January and March 1822. After bemoaning the poor health that had prevented him from establishing his molecular theory by conclusive experiments, Ampère continued:

J’en fis une, cependant, au mois de juillet 1821, qui fixa entièrement mon opinion à cet égard, quoiqu’elle ne prouve que d’une manière indirecte que les courants électriques ont lieu, dans les aimants, autour de chaque particule. Ce que cette expérience prouve directement, c’est que la proximité d’un courant électrique n’en excite point, par influence, dans un circuit métallique de cuivre, même dans les circonstances les plus favorables à cette influence. . . . C’est de cette expérience que j’ai conclu, dans le temps où je l’ai faite, que les courants électriques, dont j’admettais déjà l’existence autour de chaque particule des aimants, existaient également autour de ces particules avant l’aimantation, dans le fer, le nickel et le cobalt, mais que, s’y trouvant dirigés en toutes sortes de sens, il n’en pouvait résulter aucune action au dehors, les uns tendant à attirer ce que les autres repoussent.³⁵

Let us untangle Ampère’s conclusions. First, electric currents cannot produce other currents *of any kind whatsoever* in any other conductor, even under optimal conditions. That is, Ampère did not feel that he had shown only that it is impossible to produce a macroscopic current *par l’influence* of an existing cur-

³³ Ampère, “Lettre de M. Ampère à M. Erman, secrétaire de l’Académie royale de Berlin,” *Bibliothèque universelle des sciences, belles-lettres, et arts, faisant suite à la Bibliothèque Britannique*, 1821, 17:183–191, on p. 189; and Gaspard de La Rive, “Notice sur quelques expériences électromagnétiques,” *Bibl. universelle*, 1821, 16:201–204.

³⁴ Ampère, “Réponse” (cit. n. 12), p. 214.

³⁵ *Ibid.*, pp. 213–214.

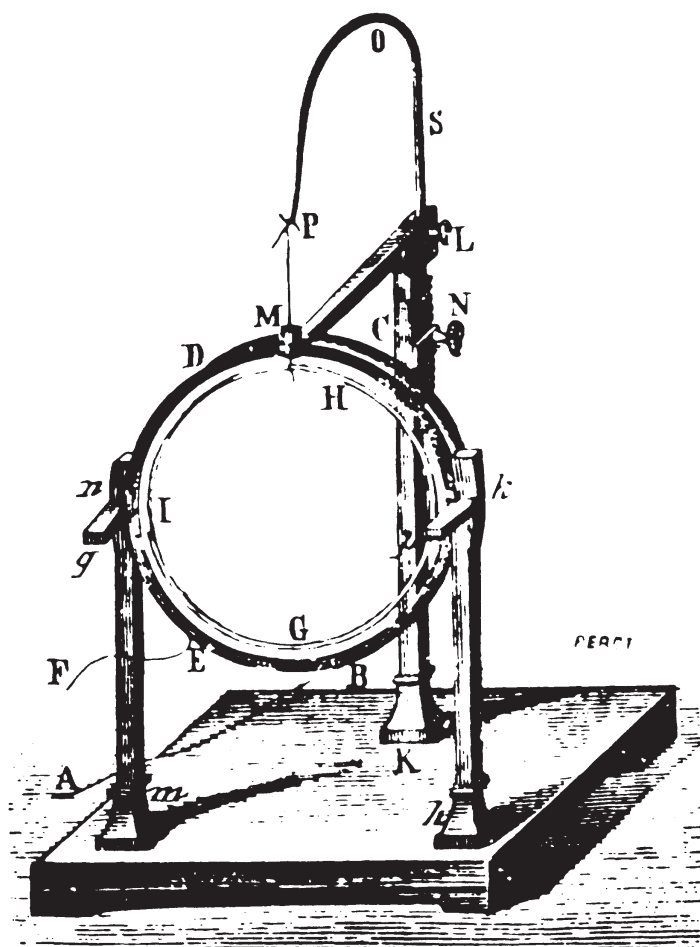


Figure 2. Ampère's induction apparatus. From "Réponse de M. Ampère à la lettre de M. Van Beck sur une nouvelle expérience électro-magnétique," Journal de Physique, 1822, 93:447–467.

rent. He believed that he had also shown that the induction of molecular currents was impossible. According to Ampère's theory, the production of molecular currents would temporarily have magnetized the copper loop; if this had happened, the loop should have responded to the bar magnet detectors by an appropriately attractive or repulsive rotation. Ampère thus claimed that the "direct" implication of the experiment was that when electric currents *do* magnetize metallic bodies, as in the method he and Arago had designed using helices, they must do so by giving a uniform orientation to preexisting currents rather than by inducing these currents. Ampère assumed that any randomly oriented molecular currents in his *copper* loop were too rigidly positioned to be given a uniform orientation by his coil.³⁶ Thus failure to magnetize the loop implied a failure to induce molecular currents. This was a dubious inference, since it presumed that Ampère's experimental conditions had been optimal for magnetization and detection. Ampère realized that subsequent use of a stronger magnet might detect induced current too weak to be revealed by his original apparatus.

Second, Ampère also claimed that "in an indirect manner" his experiment proved that the preexisting currents must be of molecular dimensions. Ampère apparently reasoned that since the experiment ruled out the possibility of induced macroscopic currents as the cause of magnetism, it also ruled out magnetization through the uniform orientation of such currents. Ampère seems to have been relying upon his prior knowledge that no effects of preexisting macroscopic

³⁶ *Ibid.*, p. 215; and Ampère to Faraday, 10 July 1822, in *Correspondance*, Vol. II, p. 587.

currents had never been detected in materials before magnetization. Nevertheless, it is possible that he also felt that if a random distribution of currents had initially been present within the suspended "cercle" of his apparatus, the surrounding coil should have given them a uniform orientation so that their cumulative effect would have been detected by his bar magnets. At least this might have been the case if Ampère's "cercle" was a cylindrical sheet of copper, as was implied by Auguste de La Rive when he described the 1822 apparatus as using "une lame de cuivre."³⁷ In short, the immediate upshot of Ampère's unsuccessful 1821 attempt to detect induced electric currents was a strong commitment to the molecular theory he had tentatively adopted in January 1821.

Although Ampère realized that a more powerful magnet might detect induced current at some future date, his trust in the molecular theory was bolstered by the explanations it provided for a wide variety of experimental discoveries announced in France by October 1821. These explanations gave Ampère considerable confirmation of the molecular theory independent of the outcome of his induction experiment. Ampère thus needed to rely upon that experiment only to discriminate in favor of the uniform alignment version of the molecular theory; its importance became increasingly restricted to a single issue between July 1821 and September 1822.

For example, in an enthusiastic article dated 7 October 1821 Albert van Beek claimed that he had produced decisive evidence in favor of the molecular theory.³⁸ In using electric discharges to alter the polarity of magnetized steel plates, van Beek discovered that this change was localized to within a limited distance from the discharge. Second, he found that a magnetized cylindrical solid manifested no magnetic effects until split into two pieces longitudinally. Both of these discoveries were to be expected according to the molecular theory, but they were puzzling for the alternative theory.

More important, however, was Ampère's response to a pivotal memoir by Michael Faraday. Besides describing the discovery of his famous "rotary effects," Faraday included a criticism of what he understood to be Ampère's latest theory of magnetism. Faraday pointed out that the poles of a bar magnet are slightly distant from rather than precisely at the ends of the magnet, as is the case when a helical conductor is used to duplicate the alleged circuitry of the magnet.³⁹ Ampère's response was to reassert his molecular theory and point out how its implications appropriately differed from those of the macroscopic theory Faraday had attacked.

In particular, Ampère's new explanation of the slightly displaced poles of a bar magnet was a striking example of how his research program drew upon a particularly powerful heuristic guideline for theory revision. Prior to his tentative adoption of the molecular hypothesis, Ampère had followed Fresnel in attributing the action of a bar magnet to the relatively large intensity of currents near its

³⁷ Auguste de La Rive, "Mémoire sur l'Action qu'exerce le globe terrestre sur une portion mobile du circuit voltaïque," *Ann. Chim. Phys.*, 1822, 21:24–48, on p. 47.

³⁸ Albert van Beek, "Sur les procédés électriques par lesquels l'acier reçoit la vertu magnétique," *Bibl. universelle*, 1821, 18:184–194.

³⁹ Faraday, "On some new Electro-Magnetical Motions" (cit. n. 13). For a recent discussion of Faraday's criticisms see L. Pearce Williams, "Why Ampère Did Not Discover Electromagnetic Induction," *American Journal of Physics*, 1986, 54:306–311; and Williams, "Faraday and Ampère: A Critical Dialogue," in *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday 1791–1867*, ed. D. Gooding and F. James (New York: Stockton Press, 1985), pp. 83–104.

center.⁴⁰ But this variation in current intensity was an *ad hoc* response to experimental observation; it was not motivated by any theoretical expectations. In the notes he and Félix Savary appended to the French translation of Faraday's memoir Ampère attempted to improve upon Fresnel's stratagem by pointing out how the mutual attraction of the current loops within the magnet would tend to make them draw together toward its center.⁴¹ This explanation at least had a theoretical basis; however, it would seem to require a corresponding variation in the density of the material molecules bearing the magnetic currents. Ampère soon realized that a more reasonable theory was to keep the location of the molecules fixed and notice that, owing to the collective interactions of current loops, the planes of all these currents would not remain normal to the magnetic axis. Instead, they should become increasingly inclined to the normal, depending upon the distance of the molecular current from the center of the magnet.

This use of variable inclinations was not available for the macroscopic theory. Furthermore, Faraday's criticism had been met simply by considering the result of a nonquantitative application of Ampère's force law to a distribution of molecular currents; there was no need to introduce additional hypotheses, such as Fresnel's variation in current intensities. Ampère thus claimed that he could have made this revision of the molecular theory without the incentive provided by Faraday's critique. In fact, Ampère extended his usual methodological rhetoric to claim that his program "could have predicted" Faraday's discoveries had not experimental detection outrun the *marche* of Ampère's theory revisions.⁴²

Aside from these defensive maneuvers, Ampère initially also had high hopes that an analysis of Faraday's rotary effects would provide conclusive confirmation of the molecular theory of magnetism. His unsuccessful exploration of this possibility soon brought about the circumstances that inspired his other two Geneva experiments. Before taking up that subject, there is a final point to be made concerning the theoretical context for Ampère's repetition of his induction experiment. Up to now I have concentrated on how the 1821 experiment was interpreted as important evidence for the preexistence of molecular currents prior to magnetization. One reason for Ampère's repetition of his experiment was his worry that originally he had not used sufficiently strong magnets as detectors.

Second, however, Ampère had also incorporated the results of the 1821 experiment into an early version of the ether mechanics that constituted the ultimate goal of his research program. In a note to the French translation of Faraday's 1821 rotary effects paper, Ampère and Savary described the negative result of the induction experiment and then added the following remarks:

Ce n'est pas, au reste, que M. Ampère n'admette qu'il peut y avoir des compositions et des décompositions d'électricité produites dans un corps conducteur par l'influence de celles d'un conducteur voisin en communication avec les deux extrémités de la pile; mais, comme elles seraient alors précisément les mêmes que dans un espace

⁴⁰ Fresnel, "Deuxième note," (cit. n. 10), p. 146; and A.-M. Ampère, "Notice sur de nouvelles recherches de Mr. Ampère," *Bibl. universelle*, 1822, 19:244–247. See also Ampère to G. de La Rive, 12 June 1822, in *Correspondance*, Vol. II, pp. 580–582.

⁴¹ A.-M. Ampère, "Notes relatives au Mémoire de M. Faraday," *Ann. Chim. Phys.*, 1821, 18:370–379, on p. 370; also in *Collection*, Vol. II, pp. 184–191, on p. 184.

⁴² Ampère to Erman, 20 Apr. 1821, and Ampère to Auguste de La Rive, 11–31 Oct. 1822, in *Correspondance*, Vol. III, p. 917, and Vol. II, p. 618.

semblable où il n'y aurait aucun corps pondérable, il n'en peut résulter des effets analogues à ceux d'un courant électrique dû à l'action électromotrice d'un élément voltaïque ou d'une particule d'aimant. Toute attraction ou répulsion produite entre deux corps par les courants électriques qui les parcourent exigent évidemment que les courants de chacun de ces corps soient produits par une cause qui réside en eux.⁴³

Ampère included these comments because during this period he was considering the possibility that his force law might be derivable from the motions of ethereal fluid. He speculated that the motions of electric fluid particles in currents isotropically produce motions in the surrounding ether. From this point of view an electric current in a conductor would experience electrodynamic forces only when the motion of the ether is different on different sides of the conductor. This would require the existence of two independent currents or the interaction of two parts of the same current. The fact that this condition was not satisfied in the induction experiment explained its negative outcome. We shall see that for Ampère the major importance of the 1822 induction experiment concerned the revisions it suggested for his ether theory. But even in this respect its importance was dwarfed by that of his other two Geneva experiments.

Both of those experiments were designed to confirm Ampère's discovery, in the spring of 1822, that the parameter k in his force law has the value $-\frac{1}{2}$. Inspired by Faraday's announcement of his first rotary discoveries, Ampère immediately investigated variations of Faraday's demonstration. He soon discovered that, regardless of what arrangement of magnets he used, as long as the potentially mobile conductor linked any two points on its axis of suspension, no rotation of the type Faraday had observed could be produced.⁴⁴

This fact suggested that he might devise conclusive proof for the molecular theory of electric currents within magnets. According to Ampère's erroneously truncated version of his electrodynamic force formula, a circular conductor should be able to bring about the rotation he had been unable to produce using magnets. Thus, if he could demonstrate that a circular coil could indeed produce the rotation in question, Ampère could argue that magnets could not contain currents of dimensions comparable to those of such a coil; otherwise, magnets should be able to produce the same kind of rotation.⁴⁵ But the expected rotation failed to materialize; Ampère found that, at most, his coil gave a slight reorientation to the suspended conductor so that it oscillated weakly and then remained motionless.

Ampère's initial reaction to this result must have been one of considerable consternation; his more studied response was to subject the state of equilibrium demonstrated by his apparatus to a detailed mathematical analysis. That is, Ampère expressed the observed state of equilibrium mathematically by applying the relevant laws of statics and the most general expression for his electrodynamic force with the values of the two parameters left unspecified. During April or May 1822 he found that he could use this equilibrium equation to derive the relation $2k = 1 - n$. His long-standing assumption that $n = 2$ thus implied that $k = -\frac{1}{2}$.

⁴³ Ampère, "Notes relatives au Mémoire de M. Faraday" (cit. n. 41), *Collection*, Vol. II, p. 190.

⁴⁴ A.-M. Ampère, "Notes sur cet exposé des nouvelles Expériences relatives aux Phénomènes produits par l'action électro-dynamique, faites depuis le mois de mars 1821," in *Recueil* (cit. n. 26), pp. 207-236, on pp. 235-236.

⁴⁵ Ampère did not discuss this argumentation in print. His reasoning is only revealed by notes preserved in Arch. Acad. Sci., Box 11, Folder 206₃.

rather than $k = 0$, as he had erroneously believed since 1820.⁴⁶ Ampère's attempt to increase the confirmation of his molecular theory of magnetism had led him to an unexpected revision of his most fundamental force law. The magnitude of this discovery is obvious; Ampère naturally was concerned to give it the strongest possible additional confirmation.

Ampère put this project into practice using both deductive and inductive modes of reasoning; the remaining two of his 1822 Geneva experiments nicely exemplify these two modes. First, from a deductive point of view Ampère scrutinized the experimental basis for his derivation of the relation $2k = 1 - n$. He soon became dissatisfied with the experiment he had used for this purpose in the spring of 1822; his analysis had relied upon the dubious claim that the apparatus remained immobile because *each element* of the circular coil exerted no effective moment on the suspended conductor. Ampère thus designed a more trustworthy replacement to be used for the first time in Geneva the following September. The original circular loop was to be replaced by a semicircular one that could be given any azimuthal orientation in the horizontal plane. Like the induction experiment, but to an even greater degree, this second Geneva experiment was designed not to explore problematic phenomena but to clarify an issue that was crucial for the continued success of his research program. The experiment was one of a carefully selected set of experiments on which Ampère eventually based his famous 1826 claim that his electrodynamic force law was "uniquement déduite de l'expérience."⁴⁷

Ampère's inductive use of experimental evidence is exemplified by the third of the Geneva experiments, which makes an instructive contrast to the induction experiment. The experiment was inspired by Ampère's realization that the new negative value of k implied that there should be a repulsive force between any pair of current elements oriented in the same direction along the line joining them. A composition of such forces thus should result in a repulsion between any two collinear segments of a linear circuit. Ampère had always considered the existence of such forces to be extremely counterintuitive; indeed, his suspicion that they did not exist had contributed greatly to his earlier belief in the erroneous version of the force law in which he had set $k = 0$. Nevertheless, in June 1822 Ampère appeared before the Académie to call attention to this implication of his newly revised formula; he later claimed to have cited it as being sufficiently surprising to merit immediate experimental investigation.⁴⁸ As will be described in the following section, Ampère had already thought of an appropriate test prior to his departure for Geneva.

⁴⁶ Ampère presented his derivation of the relation $2k = 1 - n$ at the 10 June 1822 meeting of the Académie; this initial derivation relied upon a circular coil and was published as "Mémoire sur la Détermination de la formule qui représente l'action mutuelle de deux portions infiniment petites de conducteurs voltaïques," *Ann. Chim. Phys.*, 1822, 20:398–421. By the time the memoir was published, Ampère had already performed his revised experiment with de La Rive in Geneva; he pointed out its advantages in a note on pp. 415–416.

⁴⁷ Ampère, "Mémoire sur la théorie mathématique . . . uniquement déduite de l'expérience" (cit. n. 15).

⁴⁸ Ampère, "Mémoire sur la détermination" (cit. n. 46), pp. 420–421. Ampère wrote this part of his memoir *after* the completion of his Geneva experiments. However, that he had noticed the new implications of his revised force law is recorded in the *Procès-verbaux* for the 24 June 1822 meeting of the Académie; see Académie des Sciences, *Procès-verbaux des seances de l'Académie tenues depuis la fondation de l'Institut jusqu'au mois d'août 1835*, 10 vols., Vol. VII (Paris: Hendeaye, 1916), p. 344.

IV. AMPÈRE'S THREE GENEVA EXPERIMENTS: SEPTEMBER 1822

All three of Ampère's Geneva experiments were thus designed to supply evidence required to justify, resolve, or clarify his stance on crucial issues raised by the stage his research program had reached by mid 1822. Further, Ampère's assessment and utilization of the experimental results were determined by the extent to which they fulfilled these expectations, in conjunction with his methodological priorities for the assessment of confirmatory evidence. Because the induction experiment ranks lowest according to these criteria, it is not surprising that Ampère assigned relatively little significance to it and did not undertake the exploratory research required to achieve an accurate understanding of induction.

Ampère's Second Induction Experiment

Ampère's new investigation of induction was carried out using the same experimental design he had employed in his attempt in July 1821. The only change was the replacement of the bar magnet detectors by a much more powerful horseshoe magnet provided by Marc-Auguste Pictet. As shown in Figure 2, Ampère once again suspended a circular copper loop, *HIG*, just within the circumference of a fixed surrounding circular coil, *DEB*. As shown in Figure 3, the new magnet was fixed in a stationary position prior to sending the current through the primary circuit. The effective range of the magnet was restricted to a relatively short, nearly linear, section of the loop between and adjacent to the two magnetic poles; the sole function of the magnet was to detect any induced current produced by the primary current. That is, if any appreciable macroscopic current or collection of microscopic currents was produced in the suspended copper loop, the magnet should bring about a reorientation of the loop by making it rotate away from its original position in the plane of the stationary primary coil. In September 1822 Ampère and Auguste de La Rive did observe such a reorientation as soon as they sent a current through the primary circuit. Furthermore, the suspended copper loop remained at or near its new orientation until the primary current was terminated; it then returned to its original position within the plane of the primary coil.

Recently, Eric Mendoza has accurately duplicated Ampère's experiment and verified the accuracy of his observations.⁴⁹ To avoid depletion of his power supply, Ampère probably sent a current through his coil for only thirty seconds or so. The prolonged deflection of the loop away from its initial position during a period of this length occurs because it functions as an overdamped ballistic galvanometer in the presence of the strong magnet. That is, the return to the original position is so delayed by the damping effect produced by the magnet that there is no significant return from maximum deflection throughout the relatively short period in which Ampère kept his primary current flowing. In his replication of Ampère's experiment Mendoza observed a maximum deflection of thirteen degrees. After thirty seconds this had dropped only to ten degrees; shutting off the primary current four seconds later resulted in an instant return to a position just beyond the initial state of suspension, followed by a slow return to that state.

But given that Ampère did indeed see what he reported, it is still difficult to be

⁴⁹ Eric Mendoza, "Ampère's Experimental Proof of His Law of Induction: $i_2 \propto i_1$," *European Journal of Physics*, 1985, 6:281–286.

certain how he interpreted what he saw, primarily because of the ambiguous language of the incomplete descriptions provided by both Ampère and de La Rive. Their inadequate reports indicate the scant importance Ampère ascribed to the new effect he had produced.

As soon as Ampère returned to Paris on the morning of 15 September 1822, he hurriedly composed a brief summary of his three Geneva experiments. His notes, which survive in manuscript, apparently became the text for the oral report he began presenting to the Académie the following day and completed at its next meeting one week later.⁵⁰ The surviving document includes a description of Ampère's induction apparatus lifted almost verbatim from the published version of a letter he had written to Albert van Beek early in 1822 concerning the 1821 experiment.⁵¹ Ampère simply added a paragraph to report the new result of the 1822 version:

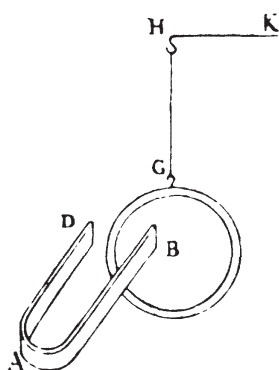


Figure 3. Diagram of Ampère's apparatus for detecting induction, used in September 1822. Based on the drawing in Ampère to Auguste de La Rive, April 1833; see La correspondance du Grand Ampère, ed. L. de Launay, Vol. II, p. 761.

Le circuit fermé placé sous l'influence du courant électrique redoublé, mais sans aucune communication avec lui, a été attiré et repoussé alternativement par l'aimant, et cette expérience ne laisserait, par conséquent, aucun doute sur la production des courants électriques par influence, si l'on ne pouvait soupçonner le présence d'un peu de fer dans le cuivre dont a été formé le circuit mobile. Il n'y avait cependant aucun action entre ce circuit et l'aimant avant que le courant électrique parcourut la spirale dont il était entouré; c'est pourquoi je regarde cette expérience comme suffisante pour prouver cette production; je me propose, néanmoins, pour prévenir toute objection, de la répéter incessamment, avec un circuit formé d'un métal non magnétique très pur. Ce fait de la production de courants électriques par influence, très intéressant par lui-même, est d'ailleurs indépendant de la théorie générale de l'action électrodynamique.

The audience could not have found this superficial gloss very informative unless Ampère embellished it with extensive explanatory comments. First, it is not clear from this passage whether Ampère was claiming to have produced a single large-scale current passing through the entire copper loop or whether by "la production des courants électriques par influence" he was referring to a collection of molecular currents. This ambiguity thus forestalls any exclusive reliance upon Ampère's seemingly more straightforward claim, earlier in his Académie

⁵⁰ Ampère's notes for his 16 and 23 Sept. 1822 reports were not published until 1885: see "Notice sur quelques expériences nouvelles relatives à l'action mutuelle de deux portions de circuit voltaïque et à la production des courants électriques par influence, et sur les circonstances dans lesquelles l'action électrodynamique doit, d'après la théorie, produire dans un conducteur mobile autour d'un axe fixe un mouvement de rotation continu, ou donner à ce conducteur une direction fixe," *Collection*, Vol. II, pp. 329–337.

⁵¹ Ampère, *Réponse* (cit. n. 12), pp. 213–214.

report, that the experiment “avait pour objet de savoir si l’on peut produire un courant électrique par l’influence d’un autre courant.”⁵²

Nor did Ampère clarify the issue in the short report he deposited in the Académie dossier for 23 September 1822:

Il s’établit dans un conducteur mobile formant une circonférence complètement fermée, un courant électrique par l’influence de celui qu’on produit dans un conducteur fixe circulaire et redoublé placé très-près du conducteur mobile, mais sans communication avec lui. J’avais tenté la même expérience au mois de juillet 1821, avec un appareil tout semblable décrit dans ma lettre à M le professeur van Beek qui a été insérée dans le journal de physique; mais, ayant probablement employé un aimant trop faible, je n’avais obtenu aucun signe de l’existence du courant électrique dans le conducteur mobile—ce qui m’avait fait rejeter dans cette lettre la production des courans électriques par influence. La dernière expérience que je viens de rapporter doit nous la faire admettre, mais ce fait, indépendant jusqu’à présent de la théorie générale des phénomènes électro-dynamiques, n’apporte aucun changement à cette théorie.⁵³

Here Ampère’s first sentence seems fairly clear, but he once again becomes ambiguous in the subsequent reference to “courans électriques.” Similarly, in the official “analyse des travaux” of the Académie compiled by Jean Fourier, Ampère’s discovery is said to have concerned “la production du courant électrique,” a carefully noncommittal phrase.⁵⁴

Although Ampère was studiously ambiguous in most publications, there is enough evidence elsewhere to indicate that he felt that he had temporarily magnetized his copper loop by inducing molecular currents. For example, Auguste de La Rive reported the outcome of the experiment in a single paragraph in an 1822 publication. The passage concludes as follows: “Cette expérience importante montre donc que les corps qui ne sont pas susceptibles, au moyen de l’influence des courans électriques, d’acquérir une aimantation permanente, comme le sont le fer et l’acier, peuvent du moins acquérir une sorte d’aimantation passagère pendant qu’ils sont sous cette influence.” Given Ampère’s molecular theory of magnetism, this passage implies an induction of molecular currents. This interpretation is strengthened by Antoine-César Becquerel’s comment in 1824 in a memoir investigating the magnetization of needles and powders. Referring to Ampère’s experiment, Becquerel claimed:

Il résulterait de cette expérience, vu l’action semblable exercée par les deux poles de l’aimant en fer à cheval sur une même portion du circuit, que l’influence du courant électrique aurait développé dans la lame un autre courant électrique, tel qu’on en observe dans un fil métallique qui communique avec les deux poles d’une pile voltaïque; mais M. Ampère s’est assuré depuis qu’il n’en était pas ainsi.⁵⁵

⁵² Ampère, “Notice sur quelques expériences nouvelles” (cit. n. 50), pp. 334, 332.

⁵³ The original text of the entire “extrait” is preserved at the Académie in the dossier for 23 Sept. 1822; the fact that Ampère read it is recorded in the *Procès-verbaux* (cit. n. 48), Vol. VII, p. 366. Ampère also published the quoted passage, without the personal pronouns, in his “Extrait d’un Mémoire lu à l’Académie royale des Sciences, dans la séance du 16 septembre 1822,” *Bulletin de la Société philomathique*, octobre 1822, pp. 145–147, on p. 146.

⁵⁴ J. Fourier, “Analyse des travaux de l’Académie royale des sciences, pendant l’année 1822: Partie mathématique,” *Mém. Acad. Roy. Sci.*, 1826, 5:231–320, on pp. 282–283.

⁵⁵ A. de La Rive, “Mémoire sur l’Action” (cit. n. 37), pp. 47–48; and A.-C. Becquerel, “Des Actions magnétiques ou actions analogues, produites dans tous les corps par l’influence de courans électriques très-énergiques,” *Ann. Chim. Phys.*, 1824, 25:269–278, on p. 272.

Although Becquerel does not provide the source of his information, it presumably came from Ampère himself, since they were collaborating closely during 1823 and 1824. In a long presentation to the Académie on 22 December 1823, Ampère interpreted Becquerel's work as demonstrating "la production, par influence, de courants électriques autour des particules de tous les corps, même les moins conducteurs de l'électricité, courants que M. Ampère avait obtenus seulement dans un cercle de métal."⁵⁶

Similarly, in an 1825 letter to John Herschel, Ampère refers to the result of his induction experiment as "petits courants électriques." Finally, in his most widely distributed published description Ampère wrote: "Un circuit métallique, continu et isolé, placé très près d'un autre circuit parcouru par un courant électrique très intense, est attiré ou repoussé par un aimant, comme s'il s'y produisait un faible courant électrique, par l'influence de l'autre circuit."⁵⁷ Here the phrase "comme s'il s'y produisait" has a counterfactual implication, although Ampère does not go on to explain his conviction that he had actually produced molecular currents.

When all this information is taken into account, it is safe to conclude that prior to Faraday's discovery, Ampère interpreted his experiment as a temporary magnetization of a copper loop due to a production of steady molecular currents as long as another steady current flows in an adjacent circuit. In this respect the experiment was clearly a disappointment for Ampère. In 1821 he had argued that, since current could not be induced by another current, the molecular currents responsible for magnetism must exist with random orientations prior to any magnetization process. In 1822 he continued to assume, as he had in 1821, that molecular currents ordinarily exist in metals such as copper but are not subject to reorientation; he thus interpreted his 1822 experiment as an induction of molecular currents rather than an alignment of preexisting currents.

Second, Ampère assumed that molecular currents existed in other metals prior to magnetization. But since his new experiment now had shown that molecular currents could also be produced by induction, he could no longer attribute magnetization completely to the alignment of existing currents; he no longer had any way to decide whether induction or alignment was an exclusive cause or whether they both contributed. This disappointing circumstance surely was one factor in Ampère's lack of interest in exploring the details of induction phenomena. Although Ampère's notoriously inaccurate memory is an unreliable guide, at least the following passages from an 1833 letter to Faraday confirm the preceding account: "Je n'avais qu'un but en faisant alors ces expériences. . . . c'était de résoudre cette question: les courants électriques, auxquels sont dues les attractions et répulsions magnétiques préexistent-ils avant l'aimantation autour des

⁵⁶ A.-M. Ampère, "Extrait d'un Mémoire sur les phénomènes électrodynamiques," published for the first time in *Collection*, Vol. II, pp. 395–410, on p. 410; for Ampère's collaboration with Becquerel, see, e.g., "Note sur une Expérience relative à la nature du courant électrique, faite par MM. Ampère et Becquerel," *Ann. Chim. Phys.*, 1824, 27:29–31.

⁵⁷ Ampère to John Herschel, Paris, 25 Oct. 1825, Royal Society of London, Hs. 1. 394; also in D. Gardiner and R. Gardiner, "Nouveaux documents sur les relations d'Ampère en Angleterre," *Bulletin de la Société des Amis d'André-Marie Ampère*, April 1966, No. 23, pp. 11–16, on p. 13 (I am grateful to L. Pearce Williams for this reference); and A.-M. Ampère, *Exposé méthodique des phénomènes électro-dynamiques, et des lois de ces phénomènes* (Paris: Bachelier, 1823), p. 11, an expanded version of a memoir of the same title in *Bull. Soc. Philomathique*, Nov. 1822, pp. 177–183, and in *J. Phys.*, 1822, 95:248–257; rpt. in Ampère, *Recueil* (cit. n. 26), 325–344 (the quoted passage is identical in all these publications).

molécules . . . ou bien sont-ils produits à l'instant de l'aimantation par l'influence des courants voisins?" Ampère then added that, following his production of induced current in 1822, "je pensai que la grande question de la préexistence ou de la non préexistence des courants moléculaires des métaux susceptibles d'aimantation, ne pouvait plus être déterminée de cette manière, qu'elle devait rester indécise jusqu'à ce qu'elle pût être résolue par d'autres moyens, et je ne mis plus autant d'importance à ces expériences que j'eus le tort de ne pas étudier plus à fond."⁵⁸

Ampère's induction experiment is thus a prime example of his general aversion to exploratory experimentation. When it failed to provide the evidence his research program required, he turned his attention elsewhere. I have mentioned Eric Mendoza's explanation of why Ampère observed a steady deflection of his copper loop throughout the activity of the primary current. Ampère himself did not discover that the induced current was produced only during changes in the primary current; on the basis of his earlier experience with current loops suspended in the earth's magnetic field, he attributed the small deflection he observed to a prolonged state of equilibrium between electrodynamic forces and a counteracting torsion in the suspension. That is, he believed the torsion of the suspension to have prevented the loop from taking an orientation normal to a line joining the poles of the magnet; similarly, the return of the loop to its initial position when the primary current was discontinued was simply attributed to the torsional force. Furthermore, Ampère merely noted that the direction of rotation of the loop varied with the direction given to the primary current. Since he thought that induction was a prolonged and continuous effect of the primary current itself, he did not discover that the direction of the induced current depends upon whether the primary current is increased or decreased.⁵⁹

None of these essential characteristics of induction was discovered prior to Faraday's more systematic research nine years later. It is tempting to conclude that Ampère could and *should* have made the full discovery of electromagnetic induction in 1822 and that he was prevented from doing so by the overly restricted theoretical issues dictated by a regimented research program.⁶⁰ I doubt that this criticism would be voiced without the hindsight provided by our present knowledge of the importance of induction. For two years Ampère's program had guided him as he systematically revised his theories to embrace a steadily expanding experimental domain. Elated by his brilliant determination of the value of the parameter k , Ampère focused his main concern in 1822 on the investigation of phenomena directly relevant to further confirmation or revision of his latest theory of magnetism. In an 1833 letter to Faraday Ampère admitted that in spite of his failure to notice that induction was a transient phenomenon, he might have used the direction of induced current as an argument for the uniform alignment

⁵⁸ Ampère to Faraday, 13 April 1833, in *Correspondance*, Vol. II, pp. 765, 766.

⁵⁹ Auguste de La Rive reported that the direction of rotation of the suspended copper loop reversed "suivant le sens du courant, dans les conducteurs environnans": see A. de La Rive, "Mémoire sur l'Action" (cit. n. 37), p. 48. This gloss was so inaccurate that some readers interpreted it as claiming that the direction of induced current was the same as that of the primary current. Jean-Baptiste Demonferrand passed along this erroneous interpretation in his influential text *Manuel d'électricité dynamique* (Paris: Bachelier, 1823), pp. 173–174. Ampère sent a copy of Demonferrand's book to Faraday, and this contributed to Faraday's inaccurate understanding of Ampère's experiment. For Faraday's recollections, see Michael Faraday, *Experimental Researches in Electricity*, 3 vols. (New York: Dover, 1965), Vol. I, pp. 23, 107–109.

⁶⁰ Williams, "Why Ampère Did Not Discover Electromagnetic Induction" (cit. n. 39), p. 311.

version of the molecular theory.⁶¹ But this would have been a most tenuous argument; discovery of the transient nature of induction obviously would have been far more reliable. Nevertheless, to pause for a detailed investigation of an effect having no clear relevance to theoretical progress understandably struck Ampère as a waste of time spent more profitably elsewhere. Admittedly, he was premature in simply concluding that induction was "independent of the general theory of electrodynamic action";⁶² but I shall argue that his decision was accelerated by the clear implications to be drawn from his other two Geneva experiments.

There was only one respect in which induction, as Ampère understood it, was at least temporarily seen to be relevant to the goals of his research program. Ultimately, Ampère hoped to show that the propagation of electrodynamic forces could be attributed to states of an ether determined by the two electric fluids. By the end of 1825 Ampère had perfected a brilliant mathematical translation between the theoretical concepts of the magnetic fluid program of Poisson and his own molecular currents. On the basis of his induction discovery, he then attributed the dynamic effects of electric currents to the induction of tiny currents or equivalent magnetic dipoles "in the surrounding space." But there is no indication that Ampère had any inkling of this model in 1822; he apparently abandoned it during 1826.⁶³ In 1822, then, the induction experiment was thoroughly disappointing from Ampère's point of view. It eroded the foundation for the argument he had constructed in 1821 for the uniform alignment version of the molecular theory of magnetism, and it did not immediately suggest any implications for Ampère's long-range project in ether mechanics.

The Equilibrium Experiment

In sharp contrast to the induction discovery, the result of Ampère's second experiment, which he designed as a basis for his derivation of the important relation $2k = 1 - n$, thoroughly conformed to his expectations. The experiment Ampère had originally relied upon early in 1822 made use of a circular coil to produce the state of equilibrium he subjected to mathematical analysis. He soon realized that his argument could be questioned because his derivation was based upon the dubious assumption that each infinitesimal component of the coil individually contributed no rotational moment to the potentially mobile suspended conductor. He then realized that he could also derive the relation $2k = 1 - n$ starting from a state of equilibrium produced by a semicircular coil. Ampère claimed that this was a more trustworthy starting point because "on ne peut donc pas, dans cette expérience, attribuer l'immobilité du conducteur suspendu, de manière à tourner librement autour d'un axe vertical, à la compensation de deux actions égales et contraires produites par deux portions du conducteur fixe semi-circulaire, puisqu'elle a lieu dans toutes les situations qu'on donne au conducteur mobile."⁶⁴

There is good reason to suspect that Ampère was bluffing here. In spite of his

⁶¹ Ampère to Faraday, 13 April 1833, in *Correspondance*, Vol. II, p. 766.

⁶² Ampère, "Notice sur quelques expériences nouvelles" (cit. n. 50), p. 334.

⁶³ A.-M. Ampère, "Extrait d'un mémoire sur l'action exercée par un circuit électrodynamique . . . lu à l'Académie royale des Sciences, le 21 novembre 1825," *Correspondance mathématique et physique des Pays-Bas*, 1826, 2:35–47; rpt. in *Collection*, Vol. III, pp. 203–216; on p. 214; see also Blondel, *Ampère* (cit. n. 1), pp. 150–154.

⁶⁴ Ampère, "Notice sur quelques expériences nouvelles" (cit. n. 50), p. 332.

assertive rhetoric he probably realized that his new experiment could be criticized on basically the same grounds as its predecessor. As he admitted in 1826, he still had actually observed only a state of equilibrium produced by his entire circuit; he had simply replaced a circular set of current elements with a less symmetric set. Nevertheless, for want of anything better, Ampère continued to rely upon this Geneva experiment during the following three years. Only in the autumn of 1825 did he design a completely different apparatus to replace it.⁶⁵

Ampère's second Geneva experiment thus was of distinctly greater importance to him than the induction discovery. The result of the experiment conformed to his prediction of a state of equilibrium, and this allowed at least a slightly more rigorous derivation of a relationship crucial to his specification of the parameters in his force law. Nevertheless, Ampère always regarded the experiment as a stopgap. Like the induction demonstration, its importance was overshadowed by that of his third Geneva experiment.

The Floating-Wire Experiment

In June 1822 Ampère had used the newly revised version of his force law to predict that there should be a repulsive force acting along the line joining any two collinear portions of a circuit. The issues at stake here were of great importance to him. First, because of the strong priority Ampère gave to successful "prediction" as confirmatory evidence, he was likely to welcome the chance to verify a particularly impressive prophecy. This was especially true in 1822. During the previous year he had been forced to insist that new discoveries should count as strong confirmatory evidence because they "could have been predicted in advance" by his latest theory.

Second, the confirmatory prediction in this instance involved the most fundamental force law of Ampère's program. The recognition that for eighteen months he had been using an incorrect value for one of this parameters had come as an embarrassing surprise; he was appropriately concerned about the credibility of his program within the intensely competitive milieu of French physics. And there certainly was no lack of active resistance to Ampère's views. Late in August 1822 Ampère's colleague Jean-Baptiste Demonferrand wrote from Paris to warn him that Claude Pouillet had appeared before the Académie des Sciences in Ampère's absence and had supported Biot's claim that the fundamental force operative in electrodynamic phenomena was a transverse force acting between a segment of electric current and a particle of magnetic fluid. Demonferrand further advised Ampère that Pouillet had made it clear that "c'est d'après cette rue théorique qu'il va continuer ses recherches sur l'action mutuelle des courans."⁶⁶ An impressive counterattack was clearly in order.

Finally, Ampère quickly realized that his successful confirmation of a repul-

⁶⁵ A.-M. Ampère, "Mémoire sur une nouvelle Expérience électrodynamique, sur son application à la formule qui représente l'action mutuelle de deux élémens de conducteurs voltaïques, et sur de nouvelles conséquences déduites de cette formule," *Ann. Chim. Phys.*, 1825, 29:381-404. In the 1826 version of "Mémoire sur la théorie mathématique" (cit. n. 15) Ampère admitted: "J'avais ensuite abandonné cette manière de déterminer la valeur de k , parce que le calcul dont je m'étais servi pour l'en déduire supposait établi relativement à chacun des élémens du conducteur circulaire ce que l'expérience démontrait seulement pour la totalité de ce conducteur": *Collection*, Vol. III, p. 176. This passage was suppressed from the version published in *Mém. Acad. Roy. Sci.*

⁶⁶ J.-B. Demonferrand to Ampère, 27 Aug. 1822, Arch. Acad. Sci., Box 9, Folder 182. Also, see C. Pouillet, "Sur les phénomènes électromagnétiques" *Ann. Chim. Phys.*, 1822, 21:77-79.

sive force between collinear current elements would allow him to solve what had been a nagging problem for his reduction of electrodynamic forces to ethereal vibrations. The importance of the experiment, therefore, accrued not only from Ampère's methodology for the assessment of evidence but also from the two components that characterize his research program. That is, the experiment was crucial to the coherence of his guiding assumption that electrodynamic forces had their origin in ethereal motions, and it confirmed an important theory revision within his program. Not surprisingly, the experiment was triumphantly highlighted during 1822 and 1823, when Ampère published several versions of an experimentally based semiaxiomatic presentation of electrodynamics.⁶⁷

Fortunately for Ampère, the experiment itself was considerably easier to describe than his investigation of induction had been. To test for a repulsion between collinear current elements, Ampère designed a circuit in which the action

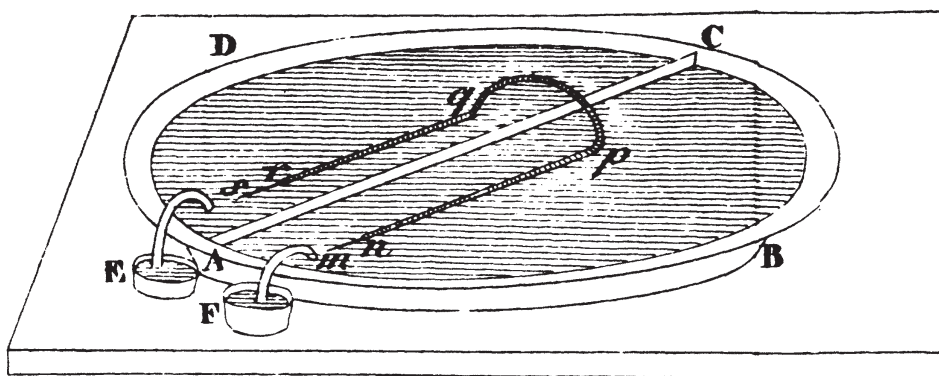


Figure 4. Ampère's floating-wire experiment. From Auguste de La Rive, "*Mémoire sur l'action qu'exerce le globe terrestre . . .*," *Annales de Chimie et de Physique*, 1820, 15:93–102.

of the forces in question would be expected to bring about a motion in a thin conducting wire floating on the surface of a shallow dish of mercury. As shown in Figure 4, the conductor *srqpm* consists of two parallel straight sections *srq* and *mnp* connected at one pair of their adjacent ends by a third wire segment *pq* of unspecified shape. The dish of mercury is divided into two independent halves by a nonconducting partition *AC*. The two linear portions of the wire float on the mercury surface on opposite sides of the partition. The entire wire is wrapped with silk insulation except near the bare ends, *s* and *m*.

An electric current is sent into the mercury from a terminal *E*, for instance, which is directly aligned with one of the linear segments of the floating wire. This current then passes through the mercury into the bare end of the wire and completes its circuit by flowing successively through the first linear segment, the connecting section passing over the partition, the second linear segment on the other side of the partition, and finally through the mercury to a second terminal where it returns to its source.

When Ampère and Auguste de La Rive put this apparatus into operation in Geneva, they discovered that as soon as a current was sent through the circuit, and regardless of the direction of this current, the originally motionless floating wire was propelled across the mercury pool away from the terminals connected

⁶⁷ Ampère, *Exposé méthodique* (cit. n. 57).

to the power source. Ampère immediately attributed this phenomenon to the action of repulsive forces between collinear pairs of current elements, that is, pairs for which one member is an element of the current in the mercury flowing between the bare end of the wire and the adjacent terminal at the edge of the mercury pool, and the other is an element of one of the linear segments of the wire. Interpreted in these terms, Ampère's experiment represented a striking confirmation of the prediction he had made to the Académie three months earlier.⁶⁸

The importance Ampère ascribed to this event was promptly reflected in the way he publicized it. In sharp contrast to his ambiguous and incomplete descriptions of induction, the text for Ampère's oral report to the Académie provides a thorough and accurate account of the floating-wire experiment. The passage concludes with an example of Ampère's characteristic enthusiasm: "Voilà donc un fait tout à fait inattendu, conclu d'avance de mes formules et vérifié ensuite par l'observation et de la manière la plus frappante."⁶⁹

Seldom had Ampère's penchant for successful prediction been so happily gratified. His revision of the value of the parameter k had been confirmed through a successful prediction of a phenomenon that had not contributed to the revision in question; for Ampère this was the strongest possible indication that his revised force law was "l'expression des véritables rapports des faits qu'elle explique."⁷⁰

But important though this confirmation was for Ampère, his subsequent citations of his floating-wire experiment were by no means limited to this issue. In his oral report to the Académie he was quick to point out that his demonstration also provided an important new explanation for the operation of a piece of apparatus typically referred to as the *moulinet électrique*, an explanation Ampère had briefly mentioned to the Académie in June.⁷¹ The *moulinet* is easily constructed using a short piece of brass or iron wire. Both tips of the wire are honed to sharp points and bent perpendicular to its length; the wire is then mounted on a pivot like a compass needle. When the originally stationary wire is given an intense electric charge through the pivot base, it begins to rotate on its pivot like a turnstile; hence the name *moulinet électrique*, or "electric mill." This effect had customarily been explained by presuming that electric fluid spreads into the surrounding atmosphere from the sharpened tips of the wire where the fluid concentration was the greatest. The observed rotation was then explained by assigning the escaping fluid a momentum of equal magnitude and opposite direction to that of the rotating wire of the *moulinet*.⁷²

This attribution of an appreciable momentum to moving electric fluid was a serious issue for Ampère. The most general guiding assumption of his program was that all electrodynamic phenomena were due to forces between pairs of

⁶⁸ Ampère, "Notice sur quelques expériences nouvelles," (cit. n. 50), pp. 330–331.

⁶⁹ *Ibid.*, p. 331.

⁷⁰ A.-M. Ampère, "Exposé sommaire des nouvelles Expériences électro-magnétiques faites par différents Physiciens, depuis le mois de mars 1821, lu dans la séance publique de l'Académie royale des Sciences, le 8 avril 1822," *Recueil* (cit. n. 26), pp. 199–206, on p. 206.

⁷¹ Ampère, "Mémoire sur la détermination" (cit. n. 46), p. 420.

⁷² During the early years of his teaching career Ampère also had applied conservation of momentum to escaping electric fluid to explain the *moulinet* in his lectures on mechanics at the Ecole polytechnique: see Arch. Acad. Sci., Box 11, Folder 209. Biot also gave this standard explanation in his authoritative textbooks: see J.-B. Biot, *Traité de physique expérimentale et mathématique* (Paris: Deterville, 1816) Vol. II, pp. 324–326; and Biot, *Précis élémentaire de physique expérimentale* (Paris: Deterville, 1817–1824) Vol. I, pp. 537–539.

moving particles of electric fluid. Since these forces were postulated to be “primitive” and irreducible to any simpler constituents, Ampère insisted that they must conform to Newton’s third law and be directed along the line joining the fluid particles. On the other hand, there was no corresponding necessity that the force expressed by his force law for “infinitesimal” current elements must act along the line connecting these elements. That is, the force between current elements need not be “central.” Indeed, since each tiny current component contained a multitude of oscillating fluid particles, the direction of these oscillations could be expected to make some contribution to Ampère’s force law. Ampère in fact took this to be the source for the angular factors in his force formula. In addition, however, if the electric fluid particles within an electric current moved with an appreciable momentum, they could impart a momentum of the same order of magnitude to the electric particles constituting the surrounding ether. If this alleged momentum of the ether was ignored, it would thus be possible for an empirically adequate expression for the force between current elements to be a noncentral force. That is, in addition to its angular factors, the force might not act along the line connecting the current elements. Nevertheless, Ampère had assumed the centrality of this force throughout the first two years of his research; he had even argued that this property indicated that his force law was a more trustworthy basis for analyzing electrodynamic processes than the noncentral Biot-Savart force being promoted by Claude Pouillet during the summer of 1822. As long as Ampère did not have to assign an appreciable momentum to moving electric fluid, he could cite the centrality of his force law as a direct consequence of Newton’s third law and, at the same time, maintain his assumption that the motion of electric fluids was the ultimate cause of electrodynamic forces. This was an important stratagem for Ampère in his debate with Biot and Pouillet; it allowed him to criticize his rivals by appealing to the French penchant for central forces so as to encourage them to abandon magnetic fluid altogether and adopt Ampère’s electric fluid program.

Ampère’s sensitivity to this issue was responsible for the dispatch with which he publicized a new explanation of the *moulinet électrique* based upon his floating-wire experiment. By ascribing the rotation of the *moulinet* to repulsive electrodynamic forces between the collinear parts of the currents linking each tip of the *moulinet* wire to the adjacent air molecules, Ampère did not have to assign a finite momentum to the particles of escaping electric fluid. He could then continue both to proclaim an electric ether of negligible mass to be the mode of propagation of electrodynamic forces and also highlight his use of central forces between current elements.

Ampère’s floating-wire experiment thus had far more important implications for his ether mechanics than the induction discovery. In an 1822 letter to Gaspard de La Rive, Ampère did grant that induction was “une chose si importante quand on veut remonter aux causes physiques des phénomènes.”⁷³ But Ampère was alluding to his attempts to decide between alternative versions of his molecular theory of magnetism. Only during a brief period in 1825 did he speculate at a deeper causal level about induction effects in the ether. At that time Ampère went so far as to ascribe the rotary effects and the repulsions between collinear current elements to forces between tiny current loops induced in the space

⁷³ See Ampère, “Mémoire sur la théorie mathématique” (cit. n. 15), pp. 99–101, 111–116.

surrounding an electric current.⁷⁴ But this was a fleeting speculation and Ampère did not repeat it in subsequent publications.

Moving to the more empirically constrained realm of force laws and magnetic circuitries, we find Ampère placing his floating-wire experiment in a particularly glaring spotlight. Here the immediate incentive was Ampère's extreme sensitivity to the fact that as a member of the mathematics section of the Académie and because of his close association with psychologists and philosophers, he was looked upon as an outsider by a considerable portion of the tightly knit French physics community. Although the publication of his memoirs was ensured by the energetic editorial efforts of influential friends such as Arago, Pictet, and Gaspard de La Rive, Ampère was discouraged that the more conservative physicists were not very receptive to his ideas.⁷⁵ Inspired by his successful trip to Geneva, Ampère quickly wrote, published, and republished a crisp expository memoir revealingly entitled *Exposé méthodique des phénomènes électrodynamiques, et des lois de ces phénomènes*.

Ampère's *Exposé* unfolds in two stages. First, he showed how his electrodynamic force law can be derived from a carefully selected set of experimental observations. Second, he argued that all electrodynamic phenomena can be attributed to the application of his force law to either macroscopic or molecular currents. In his opening comments, Ampère proclaimed that a historical presentation would not be appropriate; sufficient data were at hand to allow him "les présenter dans l'ordre qui résulte naturellement de leur dépendance mutuelle." For this task Ampère's floating-wire experiment was brought to the forefront. The first of Ampère's carefully numbered paragraphs begins as follows:

Le premier de ces faits, dans l'ordre naturel, nous paraît être celui que M. Ampère a annoncé, le 24 juin 1822, à l'Académie des Sciences comme résultant de ses formules, et qui n'a été vérifié par l'expérience qu'au mois septembre suivant; c'est la répulsion mutuelle de toutes les parties d'un courant électrique rectiligne. Cette propriété semble être dans les courants électriques la source de toutes les autres; elle lie les phénomènes qu'ils présentent à ceux qui sont produits par la machine électrique ordinaire, et spécialement à la répulsion qu'on observe dans l'expérience du moulinet électrique, entre les points de l'instrument et l'air où se répand l'électricité qui sort de ces pointes.⁷⁶

Although the *Exposé* was not the proper context for Ampère to speculate about the electric fluid motions responsible for the repulsion of collinear current elements, he seemed to imply that the floating-wire experiment offered the simplest possible manifestation of such motions. All the subsequent observations Ampère presented as the experimental basis from which his force law could be "deduced" were described as more complicated variations of this *premier fait*.

Ampère's applications of the floating-wire experiment thus conform to his characteristically strict subordination of experiment to specific confirmatory and didactic tasks set by a rigorous research program. The experiment was originally used to justify Ampère's revision of his force law; it was then lifted out of this

⁷⁴ Ampère to Gaspard de La Rive, 25 Sept. 1822, in *Correspondance*, Vol. II, p. 602; and Ampère, "Extrait d'un mémoire" (cit. n. 63), p. 214.

⁷⁵ See Ampère to Faraday, 3 July 1825, in *Correspondance*, Vol. II, p. 678; on his status as outsider see François Arago, "Ampère," in *Oeuvres complètes de François Arago*, ed. J.-A. Barral, 17 vols. (Paris: Gide & Baudry, 1854–1862), Vol. II, pp. 1–116, esp. pp. 34–35.

⁷⁶ Ampère, *Exposé méthodique* (cit. n. 57), quoting from pp. 3–4.

context and reassigned a more general didactic function as the starting point for Ampère's promotion of his force law as an explanatory basis for all electrodynamic phenomena. Ampère had no inclination to vary his experiment in the exploratory frame of mind so characteristic of Faraday; as usual, when his original trial served the purpose for which he had designed it, his interest in experimentation vanished. His creative ingenuity was spent in designing demonstrative experiments appropriate for specific purposes. He generally preferred to apply the results to as many conceptual issues as possible without tinkering with the original experimental circumstances. Only when doubts arose concerning the legitimacy of his mathematical representation of his observations would Ampère be driven to return to the experimental realm to test another carefully thought-out demonstration.

IV. CONCLUSION

Once we have explored the maze of detailed conceptual problems in which Ampère's Geneva experiments were embedded, any initial surprise about his relatively indifferent response to his induction discovery vanishes. The induction experiment simply could not compete with the floating-wire experiment according to any of the criteria by which Ampère judged the importance of experimental information. The floating-wire demonstration provided a direct confirmation of a prediction based upon the latest version of Ampère's fundamental electrodynamic force law. Electromagnetic induction was neither predicted nor prohibited by any compelling constraint within Ampère's program. Most important, Ampère did not see how induction, as he understood it, might help him resolve the question of whether the molecular circuitries of magnets exist with random orientations prior to magnetization; instead, he thought the induction discovery removed what had been his most convincing way of resolving this issue.

Second, the floating-wire experiment provided important support for Ampère's attempt to combine ether mechanics with his use of central forces between current elements. By interpreting his experiment as a demonstration of a repulsive force between collinear current elements, Ampère had a new explanation for the *moulinet électrique* and no longer had to worry about complications raised by assigning an appreciable momentum to moving electric fluid. The induction experiment had relatively little bearing on this domain; not until 1825 did Ampère think that it might contribute to his search for a causal foundation for electrodynamics in ether theory. Temporarily believing that tiny induced currents were produced in space throughout the duration of a neighboring current, Ampère speculated that mutual repulsions between such currents might be a mode of transmission of electrodynamic effects. But this hypothesis was short-lived; it did not measure up to the permanent implications Ampère drew from the floating-wire demonstration.

It is clear that as long as Ampère was convinced that induction represented neither anomalous nor confirmatory evidence for his program, he was not inspired to alter his apparatus in an exploratory spirit. In spite of numerous repetitions of the experiment during his subsequent years of teaching physics at the Collège de France, neither Ampère nor any of his associates discovered the transient nature of induction and the associated direction of the induced current. The experiment became a set piece of demonstrative apparatus, and it is easy to

imagine the “correct” result of the experiment being sternly conveyed to Ampère’s students. Any temporary failure to achieve it would be attributed to a blunder on the part of the experimenter and quickly rectified. Furthermore, this scenario applies with equal force to all of Ampère’s carefully designed demonstrations. The floating-wire experiment, for example, also involves a transient effect brought about by the initial surge of current through the apparatus. The importance Ampère attributed to his initial steady-state interpretation of the experiment made it virtually inevitable that he never would tinker with his original apparatus so as to reach a more accurate understanding of his discovery.

The obvious contrast between Ampère’s approach to experimentation and that of Faraday has been a long-standing point of inquiry for historians of electrodynamics. In his recent discussion of why Ampère failed to make Faraday’s discovery of induction, L. Pearce Williams rather ominously concludes that “there is, I think, an important lesson to be learned here.” For Williams the lesson seems to be that scientific progress would have been accelerated “if Ampère had pushed his theory aside for a moment and reported fully and accurately of his 1822 Geneva experiment.”⁷⁷ But for Ampère to have done this clearly would have required a response quite out of keeping with the deeply ingrained attitude toward experimental inquiry that made him such a different physicist from Faraday. Historians have not yet clarified the origins of this divergence.

Recent research has been directed to the religious and psychological sources of Faraday’s preference for a physics devoid of mathematics and couched in terms designed for the clear communication of simple relationships.⁷⁸ On the other hand, Ampère’s demand that intelligible physics must be expressed in rigorous mathematical language surely was due both to the impact of the French scientific environment and to the demands of his own psychological orientation to scientific investigation. French physics had become highly mathematical after the turn of the nineteenth century, and Ampère endorsed a typically French view by holding that legitimate physical concepts must be “de nature à être soumises aux procédés du calcul intégral, procédés auxquels nous croyons qu’on doit ramener toutes les questions de ce genre quand on veut s’en faire des idées nettes.”⁷⁹

Ampère seems to have been driven to sharpen this distinction both by his enthusiasm for more speculative activity in metaphysics and psychology and by his devotion to Catholicism as a spiritual mode of knowledge. But a well-forged linkage between Ampère’s physics and his highly idiosyncratic, romantic isolation within a French, Roman Catholic, post-Kantian philosophical milieu is yet to be accomplished.⁸⁰

⁷⁷ Williams, “Why Ampère did not discover electromagnetic induction” (cit. n. 39), p. 311.

⁷⁸ See, e.g., Geoffrey Cantor, “Reading the Book of Nature: The Relation Between Faraday’s Religion and his Science,” and Ryan Tweney, “Faraday’s Discovery of Induction: A Cognitive Approach,” both in *Faraday Rediscovered*, ed. Gooding and James (cit. n. 39).

⁷⁹ A.-M. Ampère, “Observation additionnelle,” appended to “Extrait fait par M. Savary du Mémoire qu’il a lu à l’Académie royale des Sciences, le 3 février 1823,” *Recueil* (cit. n. 26), p. 360.

⁸⁰ The best existing attempt to understand the psychosociological origins of Ampère’s initial research in electrodynamics is the group-grid analysis by Kenneth Caneva, “What Should We Do with the Monster? Electromagnetism and the Psychosociology of Knowledge,” in *Sciences and Cultures*, ed. Everett Mendelsohn and Yehuda Elkana (Dordrecht: Reidel, 1981), pp. 101–131.